SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

TERMS:

PER YEAR,	FOUR DOLLARS
6 MONTHS,	TWO "
3 "	ONE "
SINGLE COPIES,	TEN CENTS.

PUBLISHED AT

TRIBUNE BUILDING, NEW YORK.

P. O. Box 3838.

LONDON, ENGLAND, - - - - 150 LEADENHALL ST.

SATURDAY, OCTOBER 29, 1881.

TO OUR ENGLISH READERS.

We have received from Messrs. Deacon & Co., of 150 Leadenhall street, London, England, a standing order for a large supply of "SCIENCE," which will be forwarded weekly. We shall be obliged if our English readers will make this fact known to their friends.

THE WARNER PRIZES.

WE afforded to Professor Swift ample space in our last week's issue, to reply to our strictures on his disposition of Mr. Warner's prize for Comet *b*, 1881. Our readers have now the facts before them and can judge for themselves on the merits of this matter. For ourselves we would say that, realizing the benefits that may accrue from Mr. Warner's gifts, we are not disposed to be too critical in regard to the benefactor nor to the dispenser, and we are far from supposing that either are knowingly walking in the paths of what Professor Swift calls "crookedness." But reading Professor Swift's reply, we cannot interpret it otherwise than as a confirmation of our objections to the course he has taken.

We admitted that, in this instance, under the conditions of the Warner prizes, no claimant could justly claim the prize. We followed by asserting, that as Mr. Warner waived the special conditions and told Mr. Swift to give the $200 to the man who first saw the Comet, it was his duty to have carried out his instructions to the letter.

Professor Swift confirms the position we took on this subject; in his letter he says: "all conceded that no just demands could be made on Mr. Warner" in regard to Comet *b*. Then Mr. Warner said, "inasmuch as the Comet was such a large and brilliant one, and as so many people seemed not to have understood the conditions imposed, *he would offer a special prize of* $200 TO THE ONE WHO I, after an examination of claims, should decide HAD FIRST SEEN IT."

Now comes the *muddle*. Mr. Warner admits that *under his conditions no one can claim the prize;* and therefore offers a special prize for THE ONE WHO FIRST SAW THE COMET. And yet Professor Swift in his letter of explanation says: "*the conditions of the original prize* were, neither in this nor in any other, *to be deviated from;*" and on this account concludes that "not an astronomer in the world would have awarded it."

What can be said or done with men who are so thoroughly and flagrantly inconsistent? Mr. Warner's course throughout appears to have been thoroughly practical; he saw the difficulty in awarding this particular prize, and met it in a most liberal spirit, and had his intentions been carried out, the thanks of the community would have been the unanimous response.

Passing over Professor Swift's apparent misinterpretation of Mr. Warner's instructions, the question may be asked: could "the one who had first seen it" be named? Waiving the claim of the "1000 persons with affidavits" who claimed to have seen the Comet in the United States before its possible appearance, and the 2000 other clod-hoppers and rustics whose claims appeared to have clouded the judgment of Professor Swift, we offer a few simple facts in regard to the first discoverer of Comet *b*, which would have influenced our judgment if called upon to decide on this matter:—

We believe that the first person in the United States who saw the Comet in question, noted its position, and duly reported the fact to Professor Swift was Mr. Edgar L. Larkin, of New Windsor, Ill. If Mr. Warner, however, prefers to award the prize to the *first person* who saw the Comet, irrespective of locality, then we are advised that the following facts bear on the subject:—

Dr. Gould's name was mentioned prominently in connection with its discovery, but according to his own statement, his attention was directed to it by his assistant, Mr. Wilson. But prior to this date it had been observed by Cruls, in Brazil, and also by several English astronomers at Melbourne. It now appears that Mr. John Tebbutt, of Windsor, New South Wales, is credited as the first astronomer to get an observation of this Comet; so that if the prize is to be awarded to the first discoverer, Tebbutt appears to be the man.

The assertion in Professor Swift's letter that Mr. Warner, without consultation with any, pays the prize in certain cases, causes us some surprise, as we thought that his previous experiences hardly warranted him to decide on matters astronomical, and that he delegated the task to others.

In regard to the prize essay, we would advise Mr. Warner to postpone the time of entry until January the 1st next, which will give a reasonable time for some creditable work to be done. We would also propose that the judges be named immediately. Professor Swift says in his letter, "as to who will appoint the judges I am as ignorant as are you." Who does know? Surely Mr. Warner will not propose to decide this matter.

In making these remarks we are far from desiring to disparage the value of such prizes as those offered by Mr. Warner. We understand that Mr. E. E. Barnard, who secured the last prize, is a young man under twenty-five years of age, and a self-taught astronomer. Under very discouraging financial circumstances he provided himself with a good five-inch telescope, with which he has done excellent work. His Warner prize will be turned to good account, as he writes to inform us that the $200 will enable him to purchase a plot of ground on which to build a house for his family; we need not add that an observatory will be a leading feature in Mr. Barnard's new house.

We feel a pleasure in showing the practical good Mr. Warner is doing by providing these scientific prizes, and we trust he may continue them during the following year. Our criticism is of a perfectly friendly character and made with some regret. We have received letters from subscribers confirming our view of the case, which will remain unpublished, as we desire to close the discussion.

ON THE DISCOVERIES OF THE PAST HALF-CENTURY RELATING TO ANIMAL MOTION.

By J. Burdon-Sanderson, M. D., L.L.D., F.R.S.

[*Concluded from Page* 486.]

The living muscle of a frog is placed in a closed chamber, which is vacuous—*i. e.* contains only aqueous vapor. The chamber is so arranged that the muscle can be made to contract as often as necessary. At the end of a certain period it is found that the chamber now contains carbonic acid gas in quantity corresponding to the number of contractions the muscle has performed. The water which it has also given off cannot of course be estimated. Where do these two products come from? The answer is plain. The muscle has been living all the time, for it has been doing work, and (as we shall see immediately) producing heat. What has it been living on? Evidently on stored material. If so, of what nature? If we look for the answer to the muscle, we shall find that it contains both proteid and sugar-producing material, but which is expended in contraction we are not informed. There is, however, a way out of the difficulty. We have seen that the only chemical products which are given off during contraction are carbonic acid gas and water. It is clear, therefore, that the material on which it feeds must be something which yields, when oxidized, these products, and these only. The materials which are stored in muscle are oxygen and sugar, or something resembling it in chemical composition.

[1] Ludwig's first important research on this subject was published in 1881.

And now we come to the last point I have to bring before you in connection with this part of my subject. I have assumed up to this moment that heat is always produced when a muscle does work. Most people will be ready to admit as evidence of this, the familiar fact that we warm ourselves by exertion. This is in reality no proof at all.

The proof is obtained when, a muscle being set to contract, it is observed that at each contraction it becomes warmer. In such an experiment, if the heat capacity of muscle is known, the weight of the particular muscle, and the increase of temperature, we have the quantity of heat produced.

If you determine these data in respect of a series of contractions, arranging the experiments so that the work done in each contraction is measured, and immediately thereupon reconverted into heat, the result gives you the total product of the oxidation process of heat.

If you repeat the same experiment in such a way that the work done in each contraction is not so reconverted, the result is *less* by the quantity of heat corresponding to the work done. The results of these two experiments have been found by Prof. Fick to cover each other very exactly. I have stated them in a table[1] in which we have the realization as regards a single muscle of the following forecast of Mayer's as regards the whole animal organism. "Convert into heat," he said, "by friction or otherwise, the mechanical product yielded by an animal in a given time, add thereto the heat produced in the body directly during the same period, and you will have the total quantity of heat which corresponds to the chemical processes." We have seen that this is realizable as regards muscle, but it is not even yet within reach of experimental verification as regards the whole animal.

I now proceed abruptly (for the time at our disposal does not admit of our spending it on transitions) to the consideration of the other great question concerning vital motion, namely, the question how the actions of the muscles of an animal are so regulated and coordinated as to determine the combined movements, whether rhythmical or voluntary, of the whole body.

As every one knows who has read the "Lay Sermons," the nature and meaning of these often unintentional but always adapted motions, which constitute so large a part of our bodily activity, were understood by Descartes early in the seventeenth century. Without saying anything as to his direct influence on his contemporaries and successors, there can be no doubt that the appearance of Descartes was coincident with a great epoch—an epoch of great men and great achievements in the acquirement of man's intellectual mastery over nature. When he interpreted the unconscious closing of the eyelids on the approach of external objects, the acts of coughing, sneezing, and the like as mechanical and reflected processes, he neither knew in what part of the nervous system the mechanisms concerned were situated, nor how they acted.[2] It was not until a hundred

[1] RELATION OF PRODUCT AND PROCESS IN MUSCLE.

(Result of one of Fick's Experiments.)

Mechanical product	6670	grammemillimetres.
Its heat value	15.6	milligrammeunits.
Heat produced	30.0	"
Total product reckoned as heat	54.6	"

[2] Descartes' scheme of the central nervous mechanism comprised all the parts which we now regard as essential to "reflex-action." Sensory nerves were represented by threads (filets) which connected all parts of the body to the brain ("Œuvres," par V. Cousin, vol. iv., p. 359); motor nerves by tubes which extended from the brain to the muscles; "motor centres" by "pores" which were arranged on the internal surface of the ventricular cavity of the brain, and guarded the entrances to the motor tubes. This cavity was supposed to be kept constantly charged with "animal spirits" furnished to it from the heart by arteries especially destined for the purpose. Any "incitation" of the surface of the body by an external object which affects the organs of sense does so, according to Descartes, by producing a *motion* at the incited part. This is communicated to the pore by the thread and causes it to open, the consequence of which is that the "animal spirit" contained in the ventricular cavity enters the tube and is conveyed by it to the various muscles with which it is connected, so as to produce the appropriate motions. The whole system,

years after that Whytt and Hales made the fundamental experiments on beheaded frogs, by which they showed that the involuntary motions which such preparations execute cease when the whole of the spinal cord is destroyed—that if the back part of the cord is destroyed, the motions of the hind limbs, if the fore part, those of the fore limbs cease. It was in 1751 that Dr. Whytt published in Edinburgh his work on the involuntary motions of animals. After this the next great step was made within the recollection of living physiologists: a period to which, as it coincided with the event which we are now commemorating—the origin of the British Association—I will now ask your special attention.

Exactly forty-nine years ago, Dr. Marshall Hall communicated to the Zoological Society of London, the first account of his experiments on the reflux function of the spinal cord. The facts which he had observed, and the conclusions he drew from them, were entirely new to him, and entirely new to physiologists to whom his communication was addressed. Nor can there be any reason why the anticipation of his fundamental discovery by Dr. Whytt should be held to diminish his merit as an original investigator. In the face of this historical fact it is impossible to regard him as the discoverer of the "reflex-function of the spinal cord," but we do not the less owe him gratitude for the application he made of the knowledge he had gained by experiments on animals to the study of disease. For no one who is acquainted with the development of the branch of practical medicine which relates to the disease of the central nervous system will hesitate in attributing the rapid progress which has been made in the diagnosis and treatment of these diseases, to the impulse given by Dr. Marshall Hall to the study of nervous pathology.

In the mind of Dr. Marshall Hall the word reflex had a very restricted meaning. The term "excito-motary function," which he also used, stood in his mind for a group of phenomena of which it was the sole characteristic that a sensory impression produced a motor response. During the thirty years which have elapsed since his death, the development of meaning of the word reflex has been comparable to that of a plant from a seed. The original conception of reflex action has undergone, not only expansion, but also modification, so that in its wider sense it may be regarded as the empirical development of the philosophical views of the animal mechanism promulgated by Descartes. Not that the work of the past thirty years by which the physiology of the nervous system has been constituted can be attributed for a moment to the direct influence of Descartes. The real epoch-maker here was Johannes Müller. There can be no doubt that Descartes' physiological speculations were well known to him, and that his large acquaintance with the thought and work of his predecessors conduced, with his own powers of observation, to make him the great man that he was; but to imagine that his ideas of mechanism of the nervous system were inspired, or the investigations by which, contemporaneously with Dr. Marshall Hall, he demonstrated the fundamental facts of reflex action, were suggested by the animal automatism of Descartes, seems to me wholly improbable.

I propose, by way of conclusion, to attempt to illustrate the nature of reflex action in the larger sense, or, as I should prefer so call it, the Automatic Action of Centres, by a single example—that of the nervous mechanism by which the circulation is regulated.

although it was placed under the supervision of the "*âme raisonable*" which had its office in the pineal gland, was capable of working independently. As instances of this mechanism Descartes gives the withdrawal of the foot on the approach of hot objects, the actions of swallowing, yawning, coughing, etc. As it is necessary that, in the performance of these complicated motions, the muscles concerned should contract in succession, provision is made for this in the construction of the system of tubes, which represent the motor nerves. The weakness of the scheme lies in the absence of fact basis. Neither threads nor pores nor tubes have any existence.

The same year that J. R. Mayer published his memorable essay, it was discovered by E. H. Weber that, in in the vagus nerve, which springs from the medulla oblongata and proceeds therefrom to the heart, there exists channels of influence by which the medulla acts on that wonderful muscular mechanism. Almost at the same time with this, a series of discoveries[1] were made relating to the circulation, which, taken together, must be regarded as of equal importance with the original discovery of Harvey. First, it was found by Henle that the arterial blood-vessels by which blood is distributed to brain, nerve, muscle, gland, and other organs, are provided with muscular walls like those of the heart itself, by the contraction or dilation of which the supply is increased or diminished according to the requirements of the particular organ. Secondly, it was discovered simultaneously, but independently, by Brown-Séquard and Augustus Waller, that these arteries are connected by nervous channels of influence with the brain and spinal cord, just as the heart is. Thirdly, it was demonstrated by Bernard that what may be called the heart-managing channels spring from a small spot of gray substance in the medulla oblongata, which we now call the "heart-centre;" and a little later by Schiff, that the artery-regulating channels spring from a similar head central office, also situated in the medulla oblongata, but higher up, and from subordinate centres in the spinal cord.

If I had the whole day at my disposal, and your patience were inexhaustible, I might attempt to give an outline of the issues to which these five discoveries have led. As it is, I must limit myself to a brief discussion of their relations to each other, in order that we may learn something from them as to the nature of automatic action.

Sir Isaac Newton, who, although he knew nothing about the structure of nerves, made some shrewd forecasts about their action, attributed to those which are connected with muscles an alternative function. He thought that by means of motor nerves the brain could determine either relaxation or contraction of muscles. Now as regards ordinary muscles, we know that this is not the case. We can will only the shortening of a muscle, not its lengthening. When Brown-Séquard discovered the function of the motor nerves of the blood-vessels, he assumed that the same limitation was applicable to it as to that of muscular nerves in general. It was soon found, however, that this assumption was not true in all cases—that there were certain instances in which, when the vascular nerves were interfered with, dilatation of the blood-vessels, consequent on relaxation of their muscles, took place; and that, in fact, the nervous mechanism by which the circulation is regulated is a highly-complicated one, of which the best that we can say is that it is perfectly adapted to its purpose. For while every organ is supplied with muscular arteries, and every artery with vascular nerves, the influence which these transmit is here relaxing, there constricting, according (1) to the function which the organ is called upon to discharge; and (2) the degree of its activity at the time. At the same time the whole mechanism is controlled by one and the same central office, the locality of which we can determine with exactitude by experiment on the living animal, notwithstanding that its structure affords no indication whatever of its fitness for the function it is destined to fulfill. To judge of the complicated nature of this function we need only consider that in no single organ of the body is the supply of blood required always the same. The brain is during one hour hard at work, during the next hour

[1] The dates of the discoveries relating to this subject here referred to are as follows:—Muscular Structure of Arteries, Henle, 1841; Function of Cardaic Vagus, E. H. Weber, 1845; Constricting Nerves of Arteries, B. Séquard, 1858; Aug. Waller, 1853; Cardiac Centre, Bernard, 1858; Vascular Centre, Schiff, 1858; Dilating Nerves, Schiff, 1854; Eckhard, 1864; Lovén, 1866. Of the more recent researches by which the further elucidation of the mechanism by which the distribution of blood is adapted to the requirements of each organ, the most important are those of Ludwig and his pupils and of Heidenhain.

asleep; the muscles are at one moment in severe exercise, the next in complete repose; the liver, which before a meal is inactive, during the process of digestion is turgid with blood, and busily engaged in the chemical work which belongs to it. For all these vicissitudes the tract of grey substances which we call the *vascular centre* has to provide. Like a skilful steward of the animal household, it has, so to speak, to exercise perfect and unfailing foresight, in order that the nutritive material which serves as the oil of life for the maintenance of each vital process, may not be wanting. The fact that this wonderful function is localized in a particular bit of grey substance is what is meant by the expression "automatic action of a centre."

But up to this point we have looked at the subject from one side only.

No state ever existed of which the administration was exclusively executive—no government which was, if I may be excused the expression, absolutely absolute. If in the animal organism we impose on a centre the responsibility of governing a particular mechanism or process, independently of direction from above, we must give that centre the means of being influenced by what is going on in all parts of its area of government. In other words, it is essential that there should be channels of information passing inwards, as there should be channels of influence passing outwards. Now what is the nature of these channels of information? Experiment has taught us not merely with reference to the regulation of the circulation, but with reference to all other automatic mechanisms, that they are as various in their adaptation as the outgoing channels of influence. Thus the vascular centre in the medulla oblongata is so cognizant of the chemical condition of the blood which flows through it, that if too much carbonic acid gas is contained in it, the centre acts on information of the fact, so as to increase the velocity of the blood-stream, and so promote the arterialization of the blood. Still more strikingly is this adaptation seen in the arrangement by which the balance of pressure and resistance in the blood-vessels is regulated. The heart, that wonderful muscular machine by which the circulation is maintained, is connected with the centre, as if by two telegraph wires—one of which is a channel of influence, the other of information. By the latter the engineer who has charge of that machine sends information to headquarters whenever the strain on his machine is excessive, the certain response to which is relaxation of the arteries and diminution of pressure. By the former he is enabled to adapt its rate of working to the work it has to do.

If Dr. Whytt, instead of cutting off the head of his frog, had removed its brain—*i. e.*, the organ of thought and consciousness—he would have been more astonished than he actually was at the result; for a frog so conditioned exhibits, as regards its bodily movements, as perfect adaptiveness as a normal frog. But very little careful observation is sufficient to show the difference. Being incapable of the simplest mental acts, this true animal automaton has no notion of requiring food or of seeking it, has no motive for moving from the place it happens to occupy, emits no utterance of pleasure or distress. Its life processes continue so long as material remains, and are regulated mechanically.

To understand this all that is necessary is to extend the considerations which have been suggested to us in our very cursory study of the nervous mechanism by which the working of the heart and of arteries is governed, to those of locomotion and voice. Both of these we know, on experimental evidence similar to that which enables us to localize the vascular centre, to be regulated by a centre of the same kind. If the behavior of the brainless frog is so natural that even the careful and intelligent observer finds it difficult to attribute it to anything less than intelligence, let us ask ourselves whether the chief reason of the difficulty does not lie in this, that the motions in question are habitually performed intelligently and consciously. Regarded as mere mechanisms, those of locomotion are no doubt more complicated than those of respiration or circulation, but the difference is one of degree, not of kind. And if the respiratory movements are so controlled and regulated by the automatic centre which governs them, that they adapt themselves perfectly to the varying requirements of the organism, there is no reason why we should hesitate in attributing to the centres which preside over locomotion powers which are somewhat more extended.

But perhaps the question has already presented itself to your minds. What does all this come to? Admitting that we are able to prove (1) that in the animal body, Product is always proportional to Process, and (2) as I have endeavoured to show you in the second part of my discourse, that Descartes' dream of animal automatism has been realized, what have we learnt thereby? Is it true that the work of the last generation is worth more than that of preceding ones?

JURASSIC BIRDS AND THEIR ALLIES.*

By Professor O. C. Marsh.

About twenty years ago, two fossil animals of great interest were found in the lithographic slates of Bavaria. One was the skeleton of *Archæopteryx*, now in the British Museum, and the other was the *Compsognathus* preserved in the Royal Museum at Munich. A single feather, to which the name *Archæopteryx* was first applied by Von Meyer, had previously been discovered at the same locality. More recently, another skeleton has been brought to light in the same beds, and is now in the Museum of Berlin. These three specimens of *Archæopteryx* are the only remains of this genus known, while of *Compsognathus* the original skeleton is, up to the present time, the only representative.

When these two animals were first discovered, they were both considered to be reptiles by Wagner, who described *Compsognathus*, and this view has been held by various authors down to the present time. The best authorities, however, now agree with Owen that *Archæopteryx* is a bird, and that *Compsognathus*, as Gegenbaur and Huxley have shown, is a Dinosaurian reptile.

Having been engaged for several years in the investigation of American Mesozoic birds, it became important for me to study the European forms, and I have recently examined with some care the three known specimens of *Archæopteryx*. I have also studied in the Continental Museums various fossil reptiles, including *Compsognathus*, which promised to throw light on the early forms of birds.

During my investigation of *Archæopteryx*, I observed several characters of importance not previously determined, and I have thought it might be appropriate to present them here. The more important of these characters are as follows:—

1. The presence of true teeth, in position, in the skull.
2. Vertebræ biconcave.
3. A well-ossified, broad sternum.
4. Three digits only in the manus, all with claws.
5. Pelvic bones separate.
6. The distal end of fibula in front of tibia.
7. Metatarsals separate, or imperfectly united.

These characters, taken in connexion with the free metacarpals, and long tail, previously described, show clearly that we have in *Archæopteryx* a most remarkable form, which, if a bird, as I believe, is certainly the most reptilian of birds.

If now we examine these various characters in detail, their importance will be apparent.

The teeth actually in position in the skull appear to be

*Read before Section D., British Association for the Advancement of Science, at York, Sept. 2, 1881.

in the premaxillary, as they are below or in front of the nasal aperture. The form of the teeth, both crown and root, is very similar to the teeth of *Hesperornis*. The fact that some teeth are scattered about near the jaw would suggest that they were implanted in a groove. No teeth are known from the lower jaw, but they were probably present.

The presacral vertebræ are all, or nearly all, biconcave, resembling those of *Ichthyornis* in general form, but without the large lateral foramina. There appear to be twenty-one presacral vertebræ, and the same, or nearly the same, number of caudals. The sacral vertebræ are fewer in number than in any known bird, those united together not exceeding five, and probably less.

The scapular arch strongly resembles that of modern birds. The articulation of the scapula and coracoid, and the latter with the sternum is characteristic; and the furculum is distinctly avian. The sternum is a single broad plate, well ossified. It probably supported a keel, but this is not exposed in the known specimens.

In the wing itself the main interest centres in the manus and its free metacarpals. In form and position these three bones are just what may be seen in some young birds of to-day. This is an important point, as it has been claimed that the hand of *Archæopteryx* is not at all avian, but reptilian. The bones of the reptile are indeed there, but they have already received the stamp of the bird.

One of the most interesting points determined during my investigation of *Archæopteryx* was the separate condition of the pelvic bones. In all other known adult birds, recent and extinct, the three pelvic elements, ilium, ischium and pubis, are firmly anchylosed. In young birds these bones are separate, and in all known Dinosaurian reptiles they are also distinct. This point may perhaps be made clearer by referring to the two diagrams before you, which I owe to the kindness of my friend Dr. Woodward, of the Britism Museum, who also gave me excellent facilities for examining the *Archæopteryx* under his care. In the first diagram we have represented the pelvis of an American Jurassic Dinosaur allied to *Iguanodon*, and here the pelvic bones are distinct. The second diagram is an enlarged view of the pelvis of the *Archæopteryx* in the British Museum, and here too the ilium is seen separate from the ischium and pubis.

In birds the fibula is usually incomplete below, but it may be coossified with the side of the tibia. In the typical Dinosaurs, *Iguanodon*, for example, the fibula at its distal end stands in front of the tibia, and this is exactly its position in *Archæopteryx*, an interesting point not before seen in birds.

The metatarsal bones of *Archæopteryx* show, on the outer face at least, deep grooves between the three elements, which imply that the latter are distinct, or unite late together. The free metacarpal and separate pelvic bones would also suggest distinct metatarsals, although they naturally would be placed closely together, so as to appear connate.

Among other points of interest in *Archæopteryx* may be mentioned the brain-cast, which shows that the brain, although comparatively small, was like that of a bird, and not that of a Dinosaurian reptile. It resembles in form the brain-cast of *Laopteryx*, an American Jurassic bird, which I have recently described. The brain of both these birds appears to have been of a somewhat higher grade than that of *Hesperornis*, but this may have been due to the fact that the latter was an aquatic form, while the Jurassic species were land birds.

As the *Dinosauria* are now generally considered the nearest allies to Birds, it was interesting to find in those investigated many points of resemblance to the latter class. *Compsognathus*, for example, shows in its extremities a striking similarity to *Archæopteryx*. The three clawed digits of the manus correspond closely with those of that genus; although the bones are of different proportions. The hind feet also have essentially the same structure in both. The vertebræ, however, and the pelvic bones of *Compsognathus* differ materially from those of *Archæopteryx*, and the two forms are in reality widely separated. While examining the *Compsognathus* skeleton, I detected in the abdominal cavity the remains of a small reptile which had not been previously observed. The size and position of this inclosed skeleton would imply that it was a fœtus; but it may possibly have been the young of the same species, or an allied form, that had been swallowed. No similar instance is known among the Dinosaurs.

A point of resemblance of some importance between Birds and Dinosaurs is the clavicle. All birds have these bones, but they have been considered wanting in Dinosaurs. Two specimens of *Iguanodon*, in the British Museum, however, show that these elements of the pectoral arch were present in that genus, and in a diagram before you one of these bones is represented. Some other *Dinosauria* possess clavicles, but in several families of this subclass, as I regard it, they appear to be wanting.

The nearest approach to Birds now known would seem to be in the very small Dinosaurs from the American Jurassic. In some of these the separate bones of the skeleton cannot be distinguished with certainty from those of Jurassic Birds, if the skull is wanting, and even in this part the resemblance is striking. Some of these diminutive Dinosaurs were perhaps arboreal in habit, and the difference between them and the Birds that lived with them may have been at first mainly one of feathers, as I have shown in my Memoir on the *Odontornithes*, published during the past year.

It is an interesting fact that all the Jurassic birds known, both from Europe and America, are land birds, while all from the Cretaceous are aquatic forms. The four oldest known birds, moreover, differ more widely from each other than do any two recent birds. These facts show that we may hope for most important discoveries in the future, especially from the Triassic, which has as yet furnished no authentic trace of birds, For the primitive forms of this class we must evidently look to the Palæozoic.

THE LIMITED BIOLOGICAL IMPORTANCE OF SYNTHETIC ACHIEVEMENTS IN ORGANIC CHEMISTRY.*

By Professor Albert B. Prescott.

The solicitude shown for half a century as to the biological import of chemical synthesis arises from a misapprehension of the scope of chemical action. From all we know of chemism, it must be accepted, (1) that all the matter of protoplasm and cell is carried strictly in a state of chemical combination, but (2) it cannot therefore be accepted that chemical composition supplies the essential conditions or impulses for organization or other vital functions. The synthesis of all the chemical compounds of the living body may or may not be attainable in the laboratory, but its success would give no whit of promise for the development of organization. Chemical action is distinct from cell organization as it is from heat, cohesion, etc., and its corelations with all these forces have to await demonstration by experiment. Cell growth appears to be a necessary factor in the simple splitting of sugar into alcohol and carbon dioxide, and it may or may not be an essential factor in the chemical synthesis of proteids or of cellulose.

A gentleman of Milan, Signor Lorin, deserves high credit, for the public spirit of philanthropy he has shown in offering 20,000 francs to the municipal authorities to maintain a mortuary and post mortem room wherein the bodies of all persons dying of unexplained causes shall be rigidly examined before they are cremated.

* Read before the A. A. A. S., Cincinnati, 1881.

THE TERRA DEL FUEGIANS AT THE GARDEN OF ACCLIMATION.

The whole world has heard of the savages, who are at present exhibited at the Zoological Garden of Acclimation of Bois de Boulogne; many have gone to see them, and have been well repaid, for they present an interesting spectacle to the observer. They are seen lying or squatting about the fire kindled under the trees of the large lawn, motionless for whole hours at a time, gazing with vacant eye at the astonished crowd which presses against the railings as though they contained remarkable animals. Do they think? We cannot tell this. Do they speak? Yes, they do speak, if we can call the gutteral sounds, the cluckings which at long intervals, they exchange with each other, a language. They remain there, indifferent, having no longer in operation the only cause which can agitate them, hunger; for they are fed. It is a curious sight, but also a sad one. A man at this stage of brutishness is not wholly an animal; but he is no longer a man. The Fuegians, for that is the name which Captain Weddel gave them in 1822, and which has been applied to them since that time, inhabit Terra del Fuego. When we read in the works of travelers the description of their country, we are no longer astonished at their profound degradation.

Terra del Fuego is a mountainous archipelago, separated from Patagonia by the straits of Magellan, and formed of enormous masses of steep rocks, which leave only the coast bordering upon the straits, upon which man can settle. In the parts where the rock is not absolutely bare, a thick and impenetrable forest of beeches covers the side of the mountain, and descends as far as the sea. No animal, with the exception of some foxes and birds, inhabits this country. The climate here is horrible. The mean temperature of summer, according to King and Darwin, is 10°C., and that of winter 0.°6C. Mist is perpetual here, and tempests unceasing. Scarcely a day passes without the fall of rain, and even of snow. The habitable portion is only on the rocks of the shore. In the whole country, but a few acres of plain can be found.

For a long time these Fuegians have been known, and many descriptions of them have been given. Sebold, of Weert, who accompanied Simon, of Cord, made giants of them, eleven to twelve feet high. We see from the samples which we have under our eyes, that there is a certain exaggeration in that statement. We borrow from Orbigny the description which he gives of them; in our opinion, there is nothing to be changed in it; it is absolutely applicable to our savages.

Their head, says Orbigny, is tolerably large, their face is rounded; they have a short nose, a little broadened, open nostrils, small eyes, black and horizontal; a large mouth, thick lips, white teeth, well arranged; small ears, and the cheek bones a little prominent. They have but little beard, and this they pluck out. Their hair, like that of all the Americans, is black, long, and dull. Their body is massive, their chest large, and their bow-legs are relatively rather short. The women present the same characteristics as the men, and they will return with difficulty to the proportions exacted by European æsthetics. Their mean height is from 1.56 m., to 1.68 m.

Their language, as we have before stated, is guttural, and it has been compared by Cook to the utterance of a man who is gargling. This comparison expresses well the impression that is felt on hearing them.

The great naturalist, Charles Darwin, was able, during the many months which he passed in the country which they inhabit, to observe their habits, and he has given us a picture which, in order to be just, is not very attractive: it is from him that we borrow the particulars which follow. "Forced continually to move from one region to another, according as the resources of their settlement are exhausted, the Fuegians have no fixed abode. They construct a sort of hut by planting several branches in the ground and covering them with other branches intertwined on the side where the wind blows. Their dress consists of a piece of skin, which they carry over their shoulders, and which they pass from one shoulder to the other, according to the direction of the wind. It was necessary for ceremony to persuade the Fuegians at the Garden of Acclimation to put on a pair of drawers. Often they are completely nude. Their nourishment consists chiefly of shell-fish, and now and then of the rotten flesh of a seal or of a whale. At low tide, which may be in winter or in summer, in the night or in the day, they must get up to seek the shell-fish on the rocks; the women dive to obtain the eggs from the sea or remain patiently seated in their boats for several hours until they have caught several small fish with lines without hooks. If they happen to kill a seal, or if they happen to discover the half-rotten carcass of a whale, it is the signal for an immense feast. They then gorge themselves with the horrible food, and, to complete the feast, they eat several berries or several mushrooms which have no taste."

When the different tribes go to war they become cannibals. Besides, when in winter they are strongly pressed by hunger, they eat the old women before they do the dogs, because, they say, the latter capture otters, and the old women cannot. In this regard, it is to be regretted that they did not bring some of their dogs with them. The only domestic animals of these savages ought certainly to present a precious subject of observation.

Do these savages believe in another life, have they any rudimentary religion whatever? We are not able to pronounce on this, for it is impossible to draw any explanation from the savages themselves; they are incapable of comprehending an alternative, and we can never surely know if we understand them ourselves. All that we can say is, that each tribe or family possesses a magician whose functions have not yet been exactly defined by travelers, and that the Fuegians generally bury their dead.

It has been pretended that the family tie does not exist among them. Yet, we see in the account of Darwin that York Minster, one of the Fuegians brought back by Captain Fitz-Roy to his country, took as his wife the young girl who had accompanied him to Europe, and that the other returned Fuegian also had his wife when the expedition returned to the place inhabited by the tribe with which he had been left. Is not this a proof of the existence of a family relation, rudimentary if you wish, yet a real home among these savages.

As regards property, it is an unknown thing among them. Apart from arms and utensils, no Fuegian possesses anything of his own. If he kills a seal, it is shared among all the members of the tribe. If a present is made to one of them, he breaks it and divides the pieces. It is communism in all its beauty.

The different tribes have neither government nor chief. Each of them is, however, surrounded by other hostile tribes speaking different dialects. They are separated, the one from the other, by a neutral territory which remains absolutely deserted. The perpetual wars which these tribes have, seem to have for a cause the difficulty of obtaining food. The land is so steep that they cannot change their abode except by water; and necessity has forced them to become navigators and to build boats. Those who inhabit the shores of the Straits of Magellan pass, from time to time, into Patagonia to chase the guanacos in order to renew their clothes and their provisions. But even there they encounter enemies. The Patagonians, from whom they are distinguished by race and language, as well as by habits, pursue them with ardor, and seek to reduce them to slavery. A Fuegian slave is very highly estimated by the Patagonians, who value him among themselves, according to the quality, up to $200.

When we consider the few resources which the archi

Inhabitants of Terba del Fuego at the Garden of Acclimation in Paris (After a Photograph by Pierre Petit).

pelago of Terra del Fuego offers for the existence of man, even compared to the neighboring regions on the American continent, we ask what cause has persuaded the Fuegians to establish themselves there. To-day it is beyond doubt that these people are not negroes, as Bory Saint-Vincent believed, but that they belong to an Ando-Peruvian race which inhabits the Andes and a part of the pampas of Chili. They probably occupied, in olden times, the northern banks of the straits of Magellan, and are but a remnant of the Aucas and the Araucanos of Chili. Attacked by the Patagonians of the pampian race, not as strong and more poorly armed than their adversaries, they were obliged, at a time more or less remote, to yield the place to their redoubtable enemies and to take refuge in the inhospitable regions on the other side of the strait, where the Patagonians, detestable navigators, left them in quiet.

Then little by little have acted the forces of adaptation, which all-powerful habit, in returning their hereditary effects, have adapted the Fuegians to the climate and productions of their miserable country.

Their industry is modified in the same way, and today it is reduced to the construction of miserable boats, and to the manufacture of several weapons and utensils necessary to their sad existence. The boat built of a mass of shapeless pieces of wood, covered with canvas in the shape of the skins which they customarily employ, the boat which can be seen on the basin in the neighborhood of their enclosure, makes us shudder when we think that these savages venture in this frail machine on the agitated waters which wash their country. In regard to the collection of arms and utensils which can be seen in a neighboring shed, it indicates a certain ingenuity, but shows well to what a miserable condition these poor creatures are reduced.

These Fuegians, eleven in number, four men, four women, and three children, have been brought to Europe by M. Waalen, established for many years at Punta-Arenas, capital of Patagonia.

M. Waalen, who goes to fish for seals in the waters of Terra del Fuego, finds himself in connection with these savages. He was able, by gorging them with food, by treating them with prudence, for they are not always tractable and would be able to cause great obstructions, to induce them to remain on his ship, from which they were transshipped on a Hamburg steamer which makes the passage between Valparaiso and Europe. It was while the ship touched at Havre that M. Geoffroy Saint Hilaine, informed by a despatch, saw them and bronght them here. M. Waalen deposited in the hands of the Chilian Governor of Punta-Arenas, a sum of 12 to 15,000 francs, as security, binding himself to return these savages to their country after they had made a tour through the principal capitals of Europe.

What impression will they carry back of their sojourn among civilized people? If we are to judge of this by the Fuegians that Captain Fitz-Roy returned after a sojourn of three years in Europe, the impression will be a very fleeting one. These natives, three in number, two men, York Minster and Jemmy Button, and a young girl, Fuégia, seemed almost entirely civilized. Captain Fitz-Roy landed them in the middle of their tribes, furnished them with implements and tools of all sorts, built them a house, cleared up a corner of ground, and left them in the company of a missionary. When he returned, several months after, he found no trace of their installment, and had to take on board the poor missionary, who ran the greatest danger. Of his three pioneers, two, York Minster and Fuégia who became his wife, parted in plundering their comrade, and the latter, who had taken a wife in his tribe, became a filthy and disgusting savage, delighted with his condition, scarcely knowing how to speak English, and who showed with pride to the officers of the expedition the implements of bone and of flint which he had manufactured.

It seems, after this experience, that it is impossible to draw these savages from their debasement, and yet they have an intellectual capacity, latent, it is true, which appears superior to that of Australians. They learn languages with remarkable facility, and have a spirit of imitation carried to extremes, which ought to be utilized in order to teach them things well. The future will tell us if those who are at present in the Garden of Acclimation, will derive any profit from their sojourn among us. Our opinion is that they will be delighted at finding themselves in their own homes, and the remembrance of all that they will have seen will remain in their minds as a dream which will not perhaps be wholly agreeable.—(*Translated from La Nature.*)

ON A NEW SYSTEM OF BLOWPIPE ANALYSIS.*

By Lieut.-Colonel W. A. Ross (late R. A.)

(1) THE USE OF ALUMINIUM PLATE FOR VOLATILIZING SUBSTANCES.

Volatile metals and sulphur compounds, &c., are, in the old system, treated before the blowpipe, as is well known, upon the support of a parallelopiped of charcoal held horizontally in the direction of the blast from the blowpipe, the disadvantages of which are: (*a*) that *black* sublimates as those now known to be obtainable from arsenic, antimony, lead, &c., are undistinguishable on the black charcoal. (*b*) The greater part of the sublimate from most volatile metals is blown away by the blast—a serious objection when, as is often the case, there is only a trifling proportion of such metals present in a mineral or compound. (*c*) When the charcoal becomes incandescent, the most interesting portion of the sublimate (that next the assay) is often thus resublimed and lost. (*d*) The white charcoal ash is so mixed up with sublimates as often to conceal them, and, in cases of minute quantities, to mislead the operator into supposing there is a sublimate at all. (*e*) In the treatment of a compound containing two or more volatile metals, sulphides, or oxides, the sublimates obtained therefrom are mechanically, and perhaps sometimes chemically, combined, and then cannot be separated, so as to be distinguished from each other, by means of the blowpipe, or in any other way at the time, on the spot. (*f*) It is impossible to obtain a blowpipe sublimate from charcoal free from the silica, &c., of the ash, by scraping it off for supplementary examination. (*g*) Most charcoals, after blowpipe treatment for any length of time, split up into cracks and deep fissures, into which the sublimate or the assay falls and is lost.

Here are several objections to the use of charcoal as a blowpipe support; most of them serious, some fatal to a thorough pyrological examination of volatile substances; and yet it has obtained ever since Von Swab invented the chemical employment of the blowpipe in 1738 (in which year he thus treated an ore of zinc at Delarne in Sweden), and is still used at Freiberg.

In 1869 Napoleon III had offered, or I understood him to have offered, a premium of £1000 to any one who could discover an efficient solder for aluminium, and being then on sick-leave in India, I thought of employing my leisure in attempting this discovery.†

After investigation, I imagined (from burning my fingers so often), that the reason an aluminium solder could not be made, was the enormous heat-conducting powers of the metal, which transferred the heat from a blowpipe-flame so quickly away over the entire substance of a fragment of given bulk, that no one part of it could

*British Association, York, 1881.

† In reply to a question, Col. Ross answered that he had not discovered a new solder, but that on one occasion last year (1880) he actually did succeed in soldering two small pieces of aluminium together, and that he has a description of the process in his notes.

be raised to the fusing-point, so that, although small portions of almost every other metal or alloy could be readily fused upon it, even the most fusible, such as antimony, bismuth, &c., could not be made to combine with the aluminium.

As I had then studied blowpipe analysis on the Freiberg system for ten years, it was obvious to me that, although I had no chance of otaining the £1000, the facts thus ascertained might be utilized so as to make aluminnum plate or foil remedy in part, at all events, the disadvantages above described of charcoal as a blowpipe support.

I found that arsenic, antimony, bismuth, &c., the fusion of the smallest particle of which upon platinum is so fatal to it, could be treated without the slightest danger before the blowpipe upon aluminium, which metal also, probably from the reason above given, withstood heat concentrated upon any point, in direct proportion to the bulk of the fragment used as a support.

I found that some volatile metals, as *e. g.*, antimony, would not yield a sublimate when treated before the blowpipe upon the bare aluminium plate, but readily did so when a small slip or lozenge of charcoal was placed between the assay and the aluminium. Here, then, was a rapid and effectual means of *separating* the pyroxides or sublimates obtainable from a compound, for instance, of antimony and arsenic; the latter subliming readily upon the bare aluminium plate; the former only after treatment upon a charcoal slip.

The horizontal charcoal support was, of course, changed into a perpendicular one, in direct opposition to the blast from the blowpipe, so as to catch all sublimates of every kind; the grey-colored, shining aluminium betrayed at once the faintest sublimates, whether black or white; these, again, could be readily treated by the oxidizing or reducing flame of the blowpipe on the aluminium, where they thus afforded, in most cases, new and characteristic reactions; the perpendicular aluminium could be graduated by a scale showing the different specific gravities of sublimates by their mean ascension on the plate, unacted upon by the blast as in the case of charcoal; and finally, any portion of a sublimate could be easily and cleanly scraped off with a penknife, so as to be afterwards examined in any way desired.

Another advantage I found, referrable, I presume, to the same cause (of superior heat-conduction in the support) is that the alkaline carbonates, so often used in blowpipe analysis, as in the detection of manganese, for instance, assume, when treated before the blowpipe on aluminium, a globular shape, and that the resulting bead or ball of sodium or potassium carbonate, can be readily picked, when cool, off the plate with forceps, instead of lying in a kind of pool and sticking to the metal as they do in the case of platinum foil.

To other uses of aluminium plates, as in flattening blowpipe beads and their contents for microscopical purposes, I have not time to allude.

(2) A NEW AIR-RESERVOIR MOUTH BLOWPIPE (CALLED BY ME A "PYROGENE.")

A member of the Royal Geological Survey of England told me in Jermyn street, that he believed many geologists and mineralogists were deterred from using this important little instrument by the trouble if not difficulty of blowing, and for a long time I tried to discover some means of obviating this difficulty in vain. At last, one day, in the Zoological Gardens of London, looking reflectively at the antics of some anthropoid types of our ancestors there, I could not help feeling a kind of regret that the process of "Natural Selection" should have eventually deprived my race of the pouch under the jaw, no doubt at one time possessed by them, which would have served so admirably as an air-reservoir in using the blowpipe, and it suddenly struck me that I could partially remedy the defects of specific development in this matter, by applying an elastic air-reservoir of indiarubber to the ordinary mouth-blowpipe.

Here is the result. I have made it of a simple tube-like form, instead of the usual tapering one, as seen in Black's blowpipe, because I had to adapt it to be packed in a cigar-case like this, the only way of effecting which was to have it in a telescopic arrangement, opening and shutting thus: and this arrangement had another advantage, that, namely, of adapting the length of the instrument to the differing optical focus of differing vision.

For the jet I took Wollaston's ingenious idea of passing the stem of the blowpipe through the arm of the jet; only instead of doing that, it suited my purpose better to pass the jet through the stem of the blowpipe thus. Of course, in either case, the inserted tube must fit air-tight —an easy matter to effect. Over the throat of the mouth-piece is tied a piece of oiled silk, which acts as a valve, preventing the return of the breath into the cheeks. In this manner all difficulty in blowing is entirely removed, and even a child can use this blowpipe, because all he has to do is to blow through the valve till the air-bag is filled; then he can stop until the pressure of the blast begins to slacken, when a few more breathings will refill the bag. The blast pressure from the bag may also be increased by the operator placing it between himself and the table, and gently pressing the bag with his body, which he can easily do while using this apparatus.

I have only to add that, as you observe, the jet and air-bag fit for packing into the tube of the blowpipe itself, for which purpose there is no necessity, as in the one I have here, to make the end screw off, as all one has to do is to draw the telescopic arrangement out altogether, and, slipping in the jet and bag, to shut it up again; this, of course, would make the article cheaper. Griffin makes them (with the screw end) for, I believe, half a crown, but, of course, any ordinary mechanic could make such a blowpipe for himself for a few pence.

(3) THE PYROLOGICAL CANDLE.

I begin a brief description of this fuel with the remark that it is practically impossible for the traveller to use gas of any kind—not even petroleum gas—as fuel, on account of the difficulties of carriage. The same remark applies, but in another way, to oil of any description. A bottle of this is no doubt, easily carried, but is very apt to leak at the cork, and so to spoil any or most articles near it in a box.

Considerations of this kind led me, in 1871, to look to the modern composite candle as a substitute for the Berzelius blowpipe lamp, supplied with Plattner's Freiberg apparatus, which I had used for twelve years. The candles then used for blow-pipe operations were, indeed, in no respect different from those used for illuminating purposes. How Von Engeström, Bergmann, and the more modern pyrologists who are said to still use common candles for blowpipe work, contrived to do anything useful with them, I fail to understand. With even a small wick in the *centre* of the candle, which, of course, must be turned on one side to prevent it from stopping the blast, the heat radiation from the blowpipe-pyrocone melts the tallow or wax from that side more rapidly than the remainder of the circumference melts, so that a deep channel is soon formed, down which the fluid fuel runs, leaving the wick "high and dry." The consequence is that the pyrocone becomes "thready," from the burning of dry carbonaceous particles eliminated from the wick, and when it is cut down a mass of unconsumed tallow almost covers it at one side.

I therefore adopted the plan of having the candle made with a thick, and even double, wick, placed at one side instead of in the middle of the fuel, and in order to supply more of the latter, I had my candles made a prismatic instead of a round shape. I placed a thick collar of a good conducting metal, such as zinc, round the

edge of the candle, just under the wick, in order to conduct away and diffuse through itself the vibrations of heat. At first I had a series of these metallic collars, and proposed to remove them as the candle burned down; but I afterwards found that one or two good thick zinc collars would be sufficient.

Here is a candle from my cigar blow-pipe case which I am at present using, and another unused one, as made for me by Price & Co. of Battersea.

(4) CANDLE SCISSORS.

In Plattner's apparatus scissors are supplied for cutting the lamp-wick, which of course can also be used for other purposes, *and also* a pair of pliers for squeezing the wick together, and pressing it in any direction; these latter cannot be used, from the dirty state into which they get, for anything else. I use these two articles combined into one—*i. e.*, a pair of ordinary scissors with knobs at the end. This also goes into my cigar blow-pipe-case.

(5) ORDINARY WATCHMAKER'S PLIERS,

with a piece of wire-strapping round them, to enable them to act as holders of platinum wire supports, and they also act as the best cleaners of the wire by drawing the latter from between the pressed flat sides.

(6) TWO AGATE SLABS FOR GRINDING POWDERS.

I have here got instead, a small Freiberg agate mortar, with a pestle made from an agate pen, as I had no slabs small enough to pack away in this cigar-case.

(7) REAGENTS. BORIC ACID.

It has always seemed to me as though blow-pipe workers, or, as I call them, "Pyrologists," could no more profess to begin analytical operations by using a *salt* as reagent, than the analytical chemist could say he intended to begin his solution-work by using sodium nitrate instead of nitric acid. By employing boric acid instead of borax, therefore, in 1869, I at once obtained a series of new, very pretty, and important reactions, especially in the case of the alkaline earths, which formerly used to be the weakest part of blowpipe analysis; now, they are one of the easiest. Space and time do not allow me to describe these reactions here; and, unfortunately, I have brought no boric acid with me here in order to illustrate them; but here is a little German-silver cigar-light box in which the acid is kept, as it does not thus deteriorate. This also goes into the cigar-case.

Phosphoric acid is another of my new reagents (when I say "new," I mean that they are now 12 years old, but new in the sense that they have not been as yet generally adopted.) I use it instead of the old reagent "microcosmic salt." It affords, with several oxides before the blowpipe, new and interesting colors, as in the case of cobalt oxide, which imparts to it a very fine and pure violet instead of the ordinary blue. Of course, when a sufficient quantity of soda to form metaphosphate of sodium, or microcosmic salt after the ammonia has been driven off, has been added, the bead becomes blue, and this fact enables it to be used as an alkalimeter. It is the only reagent which requires to be kept in a stoppered bottle; and is such a powerful acid before the blowpipe that gold leaf is rapidly dissolved in it, yielding a brilliant purple bead. It affords, with iron oxide, a bead the color of watery blood. This ends the list of things packed in the cigar-case.

(8) A COMPASS IN WHICH THE NEEDLE POINTS E. AND W.

This is made by bending an ordinary magnetized needle in the centre until the points are opposite, like a lady's hairpin. It is, in fact, an ordinary horseshoe-magnet suspended, and such a magnet suspended swings E. and W. for a very obvious reason. It might prove useful in Arctic voyages, as such a needle would probably possess little or no "dip." If you bend an ordinarily magnetized needle at a right or any other angle, and suspend it from or on its centre of gravity, a line bisecting the angle will point E. and W., and it was such a needle I first made in order to find a very delicate test for traces of iron in ores. The more open or obtuse the angle, the more delicate this test is. I call it the "Equatorial Needle.' With a right-angled equatorial needle you can detect the mere trace of iron in the ore *Molybdenite*.

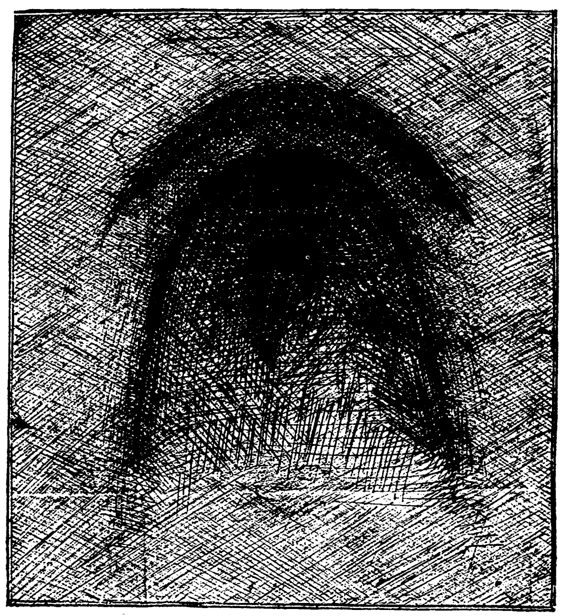
(9) AN ALLOY-BUTTON OF GOLD AND SILVER IN WHICH THESE METALS HAVE BEEN PARTLY SEPARATED BY THE BLOWPIPE ALONE.

Many years ago I found that, if you heat an alloy of two or more metals very gently with the blowpipe, so as not to promote fusion, in which case the ball spins round, and all the component metals are mixed again—that one nearly pure metal invariably leaves the others, and approaches the source of heat. This is a case of gold and silver alloy, in which the silver has approached the source of heat, but the process can be admirably illustrated in the case of a common bronze pin, in which the tin approaches the source of heat, while the copper remains in the background. Such a process might obviously be found useful in metallurgy on the large scale.

ASTRONOMY.

To the Editor of "SCIENCE."

On the early morning of June 30, 1881, the definition was very good. On no other occasion was *Comet B*, 1881, seen so clearly. As it appeared in our 8¼-inch refractor, it presented some peculiarities which I have not noticed in any published drawings, and therefore mail you the enclosed.



The prominent features were an unsymmetrical pear shaped coma surrounding the nucleus, two streams on either side, and one directly opposite the tail, which blended with the envelope. Around the whole was a very faint secondary envelope.

Very respectfully,

ISAAC SHARPLESS.

HAVERFORD COLLEGE OBSERVATORY, *September* 1, 1881.

CORRESPONDENCE.

The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

To the Editor of SCIENCE.

The "Ononid" meteors were watched this morning from 12.20 to 3.05 by four observers. The shower seemed quite abundant, 190 meteors being mapped during the time of observation. About one-half of these undoubtedly belonged to a common system. The radiant point as deduced from these, and which, considering their number cannot be greatly in error, was R. A.—86°, Dec. + 16° which brings it just outside the limits of the constellation Onon. No stationary meteors were observed and but very few with short paths near the radiant point. This may be due to the fact that they were so faint (mostly about equal in brightness to a fourth magnitude star) that the short paths were not sufficiently conspicuous to call our attention to them. An auroral light was visible in the north and east during the early part of the watch. Chambers gives 85°, +16 as the radiant point, and adds that Tupman makes it 90°, +11.

Respectfully,

ISAAC SHARPLESS.

HAVERFORD COLLEGE OBSERVATORY, PA., 10*mo*, 19*th*, 1881.

DR. H. RAYMOND ROGERS AND HIS CRITICS.

To the Editor of "SCIENCE."

Prof. Merriam, in your journal, page 495, writes as follows: "I do not like to see so great an authority as Faraday misunderstood, as he evidently is by your correspondent on page 459 of your journal, and that, too, in a way which he took particular care to caution against—as to the law of gravitating action. That it acts inversely as the square of the distance he fully believed and admitted; or, to use his own words, 'I know it is so.'"

The quotation objected to was made verbatim from Faraday's writings, and the sentiments contained therein were frequently expressed by him, and with emphasis. In the work entitled "Correlation and Conservation of Force," page 363, is an essay by Faraday entitled "The Conservation of Force," in which we read the following, viz.: "I believe I represent the received idea of the gravitating force aright in saying that it is a simple attractive force exerted between any two or all the particles or masses of matter, at every sensible distance, but with a strength varying inversely as the square of the distance. The usual idea of the force implies *direct* action at a distance: and such a view appears to present little difficulty except to Newton, and a few, including myself, who in that respect, may be of like mind with him. This idea of gravity appears to me to ignore entirely the principle of the conservation of force; and by the terms of its definition, if taken in an absolute sense, '*varying* inversely as the square of the distance,' to be in direct opposition to it." Again, in the same essay, page 366, "the assumption which we make for the time with regard to the nature of a power (as gravity, heat, etc.,) and the form of words in which we express it, that is, its definition, should be consistent with the fundamental principles of force generally. The conservation of force is a fundamental principle; hence the assumption with regard to a particular form of force ought to imply what becomes of the force when its action is *increased* or *diminished*, or its *direction changed;* or else the assumption should admit that it is deficient on that point, being only half competent to represent the force; and, in any case, should not be opposed to the principle of conservation. The usual definition of gravity as *an attractive force between the particles of matter varying inversely as the square of the distance*, whilst it stands as a full *definition* of the power, is inconsistent with the principle of the conservation of force."

Faraday is here laboring to show the incompetency of that definition *alone*. He thinks the natural philosopher ought to look for effects and conditions as yet unknown; and so virtually calls aloud for some one to fill up what to him appears a serious deficiency. He called the old definition only a *half*-assumption, and felt the necessity of some enlargement of it, that it might stand secure. He says: "the half-assumption is, in my view of the matter, more dogmatic and irrational than the whole, because it leaves it to be understood that power can be created and destroyed almost at pleasure."

Faraday called for, what we believe, the electric theory amply supplies. Not only so, but he also indicated this very source of supply. For example, a "grain of water" having a given force of gravity has also "electric relations equivalent to a very powerful flash of lightning." He says, "It may, therefore, be supposed that a very large apparent amount of the force causing the phenomena of gravitation, may be the equivalent of a very small change in some unknown condition of the bodies, whose attraction is varying by change of distance. For my own part, many considerations urge my mind toward the idea of a cause of gravity, which is not resident in the particles of matter merely, but constantly in them, and all space."

We have been led to think that it was not impossible to *find* such "cause of gravity, not resident in the particles of matter merely," but which by means of a "very small change in some [formerly] unknown condition of the bodies," shall bring the whole subject of gravitation out from the shadowy realms of darkness into abiding sunlight.

In brief, Faraday insists that the totality of the force of gravity is not expressed by the definition that "gravity acts directly as the mass and inversely as the square of the distance." Indeed, he says as pithily as when he uttered your correspondent's quotation, "I know it is so." "That the *totality* of a force can be employed according to that law *I do not believe!*"

It might, by the way, be of interest to learn a little more definitely as regards what it was that Faraday knew was so. The following are his words: "That the result of one exercise of a power may be inversely as the square of the distance I believe and admit; and I know it is so in case of gravity." The same sentence, however, continues: "but that the *totality* of a force can be employed according to that law I do not believe either in relation to gravitation or electricity or magnetism, etc."

It may be asked what can be correctly known of the action of electricity or magnetism where the item *polarity* is left out? "What I object to," says Faraday, "is the pretence of knowledge which the definition sets np when it assumes to describe, not the partial effects of the force, but the nature of the force as a whole."

Satisfied with the old definition as your correspondent may be, Faraday looked for a "missing link." We may say that he pointed it out in saying:—"when we remember that the earth itself is a magnet, pervaded in every part by this mighty power, universal and strong as gravity itself, we cannot doubt that it is exerting an appointed and essential influence over every particle of matter, and in every place where it is present. What its great purpose is seems to be looming up in the distance before us:—the clouds which obscure our mental sight are daily thinning, and I cannot doubt that a glorious discovery in natural knowledge and in the wisdom and power of God in the creation is awaiting our age."

I would conclude this part of my reply to your correspondent, with the recommendation that he study Faraday, for "I do not like to see so great an authority as Faraday misunderstood."

Again, as regards the earth's return from aphelion to perihelion:—

It is admitted that my reply (p. 459) to Mr. Hendricks

was left open to objection. This may be accounted for by the fact that there was shown to me his article *minus two paragraphs*—the last paragraph on page 458 and first on 459. Therein *inertia* alone was represented as *bringing back* the planet from aphelion to perihelion. That the planet, traveling its orbit from perihelion to aphelion, as it were *diagonally against* the central attraction of the sun, would find its velocity and momentum diminished sufficiently to be made to return, I do not doubt; but that on the *second* round, it would reach the farthest limits of its first round, I do not think there is any reason to believe. The tendency would be to bring the orbit into a perfect circle very speedily. In the polarity, which is a factor of magnetism, we find a needed *regulative* agency. Do we say that this agent is too insignificant? Nevertheless may it not be, in the words of Faraday, the "*very small change* in some unknown condition of the bodies" involved in the operation, which is all-sufficient for what is required of it? We recognize the force of Faraday's objection to the popular definition of gravity, viz.: that *alone* it is incompetent, and contradicts the law of conservation—except as we add to it *something more*. That something more we fully believe to be that electrical or magnetic constituent which Faraday says "exerts an appointed and essential influence over every particle of matter."

QUERY.—Is it wise or philosophical to recognize a cosmical force of incalculable energy, and yet in our theory of the cosmas make no practical account of it whatsoever?

H. RAYMOND ROGERS.

DUNKIRK, N. Y.

The death of Mr. Charles A. Spencer, of Geneva, N. Y., has caused universal regret, and in many respects it may be considered a national loss, for as a representative of America's skilled opticians his position was unique. As a pioneer he was the first to manufacture Microscope objectives in the United States, and at once developed a skill in the manufacture of these minute and delicate glasses, which he maintained to the last. Spencer was no copyist, his inventive genius and thorough knowledge of the optical principles involved in the making of objectives enabled him to keep in the van of all those who devoted themselves to the same art.

The greatest triumph of Spencer was in the enlargement of the angle of aperture of his objectives, in which respect he was always in advance of the best European makers, but he will always be remembered as a conscientious worker, who never permitted an objective to leave his hands which was not worthy of the maker.

EXCHANGES AND WANTS.

METEOROLOGICAL REPORT FOR NEW YORK CITY FOR THE WEEK ENDING OCT. 22, 1881.

Latitude 40° 45′ 58″ N.; Longitude 73° 57′ 58″ W.; height of instruments above the ground, 53 feet; above the sea, 97 feet; by self-recording instruments.

OCTOBER.	BAROMETER. Mean for the day. Reduced to Freezing.	Maximum. Reduced to Freezing.	Maximum. Time.	Minimum. Reduced to Freezing.	Minimum. Time.	THERMOMETERS. Mean. Dry Bulb.	Mean. Wet Bulb.	Maximum. Dry Bulb.	Time.	Wet Bulb.	Time.	Minimum. Dry Bulb.	Time.	Wet Bulb.	Time.	Maxi'm In Sun.
Sunday, 16	30.116	30.218	10 p. m.	30.000	2 a. m.	68.0	63.0	76	2 p. m.	67	2 p. m.	62	12 p. m.	56	12 p. m.	138.
Monday, 17	30.104	30.212	0 a. m.	29.918	12 p. m.	63.0	60.3	66	12 p. m.	65	12 p. m.	59	4 a. m.	54	4 a. m.	105.
Tuesday, 18	29.760	29.918	0 a. m.	29.678	2 p. m.	67.0	62.6	78	2 p. m.	68	2 p. m.	51	12 p. m.	48	12 p. m.	131.
Wednesday, 19	30.092	30.118	11 a. m.	29.908	0 a. m.	50.0	45.6	55	2 p. m.	4[illegible]	2 p. m.	45	6 a. m.	44	7 a. m.	120.
Thursday, 20	30.031	30.098	0 a. m.	30.000	2 p. m.	51.3	49.3	56	3 p. m.	52	3 p. m.	47	1 a. m.	45	1 a. m.	125.
Friday, 21	30.179	30.200	9 a. m.	30.068	0 a. m.	53.7	50.3	61	3 p. m.	55	5 p. m.	44	7 a. m.	44	7 a. m.	128.
Saturday, 22	30.142	30.208	9 a. m.	30.100	5 p. m.	57.0	52.7	67	4 p. m.	59	5 p. m.	46	7 a. m.	46	7 a. m.	122.

Mean for the week .... 30.060 inches.
Maximum for the week at 10 p. m., Oct. 16th .... 30.218 "
Minimum " at 2 p. m., Oct. 18th .... 29.678 "
Range .... .540 "

	Dry.		Wet.	
Mean for the week	58.5 degrees		54.8 degrees.	
Maximum for the week, at 2 pm. 18th	78. "	at 2 pm 18th,	68.	"
Minimum " " 7 am. 21st	44. "	at 7 am 21st,	44.	"
Range " "	34. "		24.	"

OCTOBER.	WIND. Direction. 7 a. m.	2 p. m.	9 p. m.	Velocity in miles. Distance for the Day.	Force in lbs. per sqr. feet. Max.	Time.	HYGROMETER. Force of vapor. 7 a. m.	2 p. m.	9 p. m.	Relative humidity. 7 a. m.	2 p. m.	9 p. m.	CLOUDS. Clear, 0; Overcast, 10. 7 a. m.	2 p. m.	9 p. m.	RAIN AND SNOW. Depth of rain and snow in inches. Time of Beginning.	Time of Ending.	Duration. h. m.	Amount of water	OZONE. 0–10
Sunday, 16	w. s. w.	n. n. w.	n. e.	178	2½	1.50 am	.542	.542	.451	94	60	73	0	7 cir. cu.	3 cu.	-----	----	-----	--	3
Monday, 17	e. n. e.	e. n. e.	s.	138	1¼	6.00 am	.412	.497	.562	77	83	94	8 cir. cu.	10	10	-----	----	-----	--	0
Tuesday, 18	s. e.	w. n. w.	n. n. w.	240	12¼	11.00 pm	.612	.550	.404	89	57	93	8 cu.	9 cu.	0	-----	----	-----	--	6
Wednesday, 19	n. n. w.	n. e.	e. n. e.	234	6¼	5.50 am	.262	.243	.247	84	56	71	0	6 cir. cu.	8 cu.	-----	----	-----	--	0
Thursday, 20	n. e.	e. n. e.	s. s. e.	111	1½	2.30 pm	.284	.349	.348	85	80	93	9 cu.	5 cir. cu.	0	-----	----	-----	--	0
Friday, 21	n. w.	n. e.	s. w.	65	¼	1.15 pm	.288	.297	.391	100	55	87	0	2 cir. cu.	0	-----	----	-----	--	2
Saturday, 22	w. n. w.	s. w.	s. w.	134	2¼	3.20 pm	.311	.330	.396	100	53	76	0	0	0	-----	----	-----	--	0

Distance traveled during the week .... 1,100 miles.
Maximum force .... 12¼ lbs.

Total amount of water for the week .... .00 inch.
Duration of rain .... 0 hours, 00 minutes.

DANIEL DRAPER, Ph. D.

Director Meteorological Observatory of the Department of Public Parks, New York.